In *Modelling the small-scale deposition of snow onto structured Arctic sea ice during a MOSAiC storm using snowBedFoam 1.0* the authors present a new software package, snowBedFoam. This software is a snow transport model that uses the CFD package OpenFOAM.

In general the manuscript reads well, but could do with a few clarification passes.

My first concern is the lack of consideration for a sublimation flux. Sublimation of blowing snow and of snow packs has been identified as a major contributor to mass loss in many environments (Mott 2018). The authors do not describe the humidity of this location. Indeed, perhaps it is sufficiently humid that such an approximation is warranted. However, this is never described nor justified in anyway. I see that the MOSAiC companion paper Wagner (2021; TCD) notes low sublimation fluxes for this period, perhaps 6%. This seems derived from modelling studies and not observations. The simulation period is short and perhaps sublimation is negligible. However, that has not been demonstrated and to wave it away, especially when the model is noted to have deviations from observations, seems problematic. I would like to see a sublimation sink added and evidence that, in a distributed context that this process is indeed negligible.

The authors extensively cite Bagnold 1941 without ever noting that it is a sand transport study. I would encourage the authors to cite the vast body of literature surrounding the early work of adopting this work for blowing snow such as, e.g., Budd, 1966; Schmidt, 1982; and Pomeroy, 1990.

My second concern is shown in Fig 5. Specifically the long, low velocity streaks of $u^*$. Ivanell (2018) and Wagenbrenner (2019) have a discussion of similar streaks in RANS
models. These areas may have substantial impacts on blowing snow simulations and deserve attention. I would like to see, at a minimum, a description and placement in the literature of these features and if the authors think they are real. Wagenbrenner (2019) identifies that they are somewhat dependent upon the upwinding scheme used. Do the authors think that is the case here?

This brings me to my last concern – a lack of uncertainty analysis. I would like to see the authors quantify the impact of any meteorological forcing, e.g., their input precipitation and the values in Table 2. Certain values can have massive impacts, e.g., friction velocity threshold, and it would be useful to understand how sensitive the model is to these parameters. It is not 100% clear to me how the simulations were done. It seems to be developing a steady-state (?) simulation of 1000 s at which point the wind speed is reduced? How sensitive are the results to this (spin up?) 1000 s period?

The large temporal periods over which the simulation is run from mean values of wind is concerning or at least requires further discussion. The temporal scales that impact blowing snow are quite small, < 1 s (Aksamit & Pomeroy, 2016, 2018), although at 15 m to 1 hr scales, mean shear-stress models tend to be successful. However it is not clear how successful a many-hour mean wind structure is for representing these features. To me it seems a mis-match to run a sub-metre spatial model, but drive it with many-hour mean windflow that we know doesn't represent any of the wind structure known to drive blowing snow events.

In summary I would recommend this for major revisions. It has the potential to be a unique contribution to the blowing snow literature, however I do not believe to be there in its current form.

References


Page 1

L1 > “In these” This sentence is not clear

L2 > has a substantial effect

On what?
a large part of the snow

Of the snow mass? Clarify what this means

which complicates precipitation estimates.

Oh so this is precip undercatch?

to remove these uncertainties

It’s not clear what these are. Do you mean surface precip?

on a piece of MOSAiC sea ice

MOSAiC domain?

On a piece of

terrestrial laser scan observations?

Could help to better constrain precipitation estimates

This is not clear from the abstract why this is the case and it remains un answered.

seems to respond

Word choice for respond
Significant reduction?

Also, Arctic precipitation estimates could be significantly improved by an accurate assessment of the snow deposition on sea ice.

How? It is well known that snow depth on the ground doesn’t equal snowfall due to sublimation.

Connecting the snow mass balance to snowfall.

What about sublimation?

That is

Influence where? Certainly these have been well constrained in many studies.
To our knowledge, such spatial observations of snow deposition are non-existent in the literature for Arctic sea ice.

Is this not a summary of Trujillo 2016? Please clarify

which we perform in this study.

Page 3

spatial variability of snow deposition around complex terrain

To be clear you mean blowing snow transport?

is not yet fully understood Grünewald et al., 2010)

Is there a more recent reference?

Multiple model approaches exist that try to describe it

If the previous section is indeed blowing snow then this is missing many references. But what is ‘it’ referring to?

on a piece of sea ice

Is this the technical definition? Please be more specific

Several data sets

Datasets of what?
MOSAiC (Multidisciplinary Drifting Observatory for the Study of Arctic Climate)

Define this earlier.

The first one

Data set

This article

Consider“manuscript”

second stage,

“Section”

I would like to see scientific questions and hypothesis testing!

Page 4

data sets employed here

“Used” here

employed

that were successively operated
Do you mean observed?

L 101 > was placed as high as possible

How high is this?

L103 > were recorded relatively

Relative

L103 > Details about the use of TLS for sea ice measurement

Does that describe this dataset or in general? please clarify.

L 104 > and interpolated

Spatially? What method?

L 104 > Several correction

To the interpolated or the raw point clouds?

L 105 > is a last step, the point clouds were aggregated

Is this the QGIS step?

L 110 > with a constant snow density value of 210 kg.m−3
Was this an in situ observation on the ground or fresh snowfall?

L 111 > spatial variability of snow density

Please note what processes impact this ie is this snow compaction or from blowing snow?

Page 5

Fig 1 > during a helicopter flight

This is the first mention of a held. I thought this TLS was from the ship? Please clarify or remove.

L 120 > correction of orientation

I don’t understand what this means. Please clarify what corrections were done

L 122 > among other instruments

Please list the source of all observations.

L125 > that this data

This = Radar derived precip estimates in general or the data used here?

L127 > by releasing particles

Are all the particles same size? I think it is noted they have a distribution later but just note it here.
Page 6

L 147 > walls

I know what you mean but I would like you to clarify what you mean walls in a natural context to aid a non-domain modeller in reading this manuscript.

Page 7

L179 > which was added within the core code

By the authors? Ie is this part of the scientific contribution?

Page 8

L188 > (Bagnold, 1941)

Is this really the best reference? Bagnold 1941 is sand processes, not snow.

There are more recent descriptions of the blowing snow transport outside of wind tunnels such as:


that should be referenced.

L195 > Bagnold, 1941)
Bagnold 1941 is sand, not snow. Odd to include it in this list. Would suggest replace it with the early efforts to adopt Bagnold to blowing snow that I list above.

L200 > a threshold value defined as (Bagnold, 1941)

It is not clear to me why a sand-grain threshold is used. Where is the constant A from? Is this a snow value or a sand value?

L 207 > empirical parameter set to 1.5 (Doorschot and Lehning, 2002),

I had assumed equation (11) was referring to eqn (8) in Doorschot and Lehning, (2002) but they note a value for $C_{ae}=1$. I don’t see any other $C_{ae}$ in that manuscript so please double check this value.

Page 9

L 235 Note in this section the distribution and mean cell size of the hexes

Page 10

L 243 > with STereo Lithography (STL) input data

I know this is a file format but I think this should be clarified

L245 > the vertical grid spacing $dz$ ranges between

Are you using log spacing? Alternatively how were the vertical layer thicknesses decided?

L 255 > corresponding cell of the connected periodic plane
Maybe note this results in snow being added on the upwind domain boundary.

I would describe this for the reader who is interested in this approach but does not know what a wall means in this numerical context.

Null = zero in this context? If so just say zero for clarity

Define what you mean by neutral flow – I assume neutral stability?

Page 11

Page 12

1000 s

“were” not got
This is not clear to me exactly what this mean. Please clarify what 'deactivation' means for a wind velocity. Are developing a particle steady-state and then turn the wind off and let it settle?

Preferential deposition arises due to terrain impacts on local meteorological conditions, causing increased deposition on the leeward slopes and decreased deposition on the windward, and is typically a critical process in mountain terrain (e.g., Gerber et al., 2019; Mott et al., 2014; Vionnet et al., 2017). A few places in the manuscript it seems like blowing snow process and the deposition of suspended snow to be called 'preferential deposition'. I would like to see this tightened up so the reader is not potentially confused.

280 m, but also what is a range gate?

Ah, so this is where A is defined. Please add this to the description of eqn (10).

It is not totally clear to me not why simulate the whole time series? I realize 'compute' is offered up, but how prohibitively long would it be? It's probably outside the scope of this project (but perhaps not) however it would be interesting to know how much worse these assumptions made the model output v. running the model for the entirety of the observation period.

Figures 4 and 5 report the results
Legend needs units (even if it's in the caption)

L327 > extrapolated areal

What does “extrapolated” mean in this context?

Page 15

L336 > in blue in the surface friction velocity plots (Fig. 5).

These low friction velocity streaks require more description and a quantification if the authors believe they are ‘real’.

Page 16

L369 > Quantitatively, our model appears only partially successful

I would like to see RMSE + CV for the domain.

L 387 > may be multiple reasons for the

Density assumption is not addressed here.

Page 17

L391 > used four averaged values for wind speed and direction in OpenFOAM to represent a one-week period of measurements.
I am not surprised this didn’t when we know blowing snow is at higher temporal scale!

L392 > many specific wind conditions

Would be good to muse on if the neglected conditions are similar to those shown by Aksamit 2016, for instance. I would also explicitly note “specific wind conditions” to include, e.g., gusts, etc.

L 420 >

Could some of this be due to ignoring sublimation? Figure 6 (left) suggests an overestimation of deposition in areas such as due north of the middle of B, on the flatter (?) section. The elevation (?) isolines between these two figures are different though, making a qualitative comparison difficult.

Page 18

L425 > tendency to overestimate precipitation

By how much, exactly?

Page 20

L496 > snow distribution patterns were accurately captured

You note the following in the results section “we observe that the quantitative performance of our o model is not optimal” and Fig 6 shows substantial differences.

L498 > enhanced deposition

Blowing snow deposition, correct?
L 505 > performance

Word choice – ensure it is clear you mean the accuracy of the model output and not the computational performance of the model

Page 26

I would like to see units on the colorbars

Page 28

extrapolated snowBedFoam 1.0

I don’t understand what extrapolated means for these results